

Human Population Genetics in India. Edited by L. D. SANGHVI, V. BALAKRISHNAN, H. M. BHATIA, P. K. SUKUMARAN and J. V. UNDEVIA. Proceedings of the First Conference of the Indian Society of Human Genetics, vol. 1. New Delhi: Orient Longman Ltd., 1974. Pp. 288. \$22.50.

This is the first of three volumes reporting the Proceedings of the First Conference of the Indian Society of Human Genetics. The first two are concerned with population genetics and the third with cytogenetics and clinical genetics. This first volume contains genetic data on Indian populations and some theoretical studies, while the second will be devoted to inbreeding and multidisciplinary population studies. The genetic data are divided into biochemical polymorphisms including blood groups, Gm, HL-A, and some enzymes, but are primarily devoted to abnormal hemoglobins and the G6PD deficiency, and more classical genetic traits including anthropometrics, hand clasping, hairy ears, and age of menarche. Although some of the papers are simply reviews of published data, others present original data on genetic variation in India, and some report interesting associations with malaria (hemoglobin S and G6PD deficiency) and tuberculosis (Rh—).

The large human populations in India, their intricate interrelations, and their diverse ecological settings make it a fruitful area for future genetic investigation; a beginning is represented in this volume. With the complex population structure in India, one can understand why most of the analytical chapters are concerned with genetic distance and the relationships among populations. The biochemical data point up the extraordinary genetic variability found among Indian populations, however, the variability itself varies considerably among the loci investigated. This raises problems for the current emphasis on genetic drift and neutral mutation as the explanation of most genetic variation. Further data from Indian populations may contribute significantly to this central issue in genetics. In fact, the data presented in this volume on the distributions of the Bombay phenotype and the new In^a blood group antigen shows that they are widely distributed all over India in rather low frequencies but are not found in similar frequencies elsewhere. This seems to raise problems for current theory.

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Race Differences in Intelligence. By J. C. LOEHLIN, G. LINDZEY and J. N. SPUEHLER. San Francisco: W. H. Freeman, 1975. Pp. 380. \$12.00.

There can hardly be a reader of this journal who is not aware that in the last 5 years there has been a major rejuvenation of an old form of biological determinism, asserting that people owe their different positions in society to differences in their genes. The argument, in brief, is that high social and economic status depends upon cognitive ability, that cognitive ability is well measured by a variety of psychological tests lumped under the heading of "I.Q. tests," and that differences in this measured ability arise mostly from genetic differences both between individuals and groups. The older form of the argument, pushed during the first quarter of this century by the American pioneers of I.Q. testing (Goddard, Terman, and Yerkes), was that immigrant groups, especially those from southern and central Europe, were genetically inferior to the northern European stock from which the I.Q. testers themselves had sprung. The newer form of the argument, no longer directed against eastern European Jews who, after all, now constitute

a large fraction of American intellectuals (especially psychologists!), is the assertion that American blacks are genetically inferior to whites, and the inferior social and economic position of blacks is the natural outcome of their biological inferiority as a group, despite supposed equality of opportunity.

A great many books, articles in scholarly journals, and journalistic pieces have appeared on the subject of genetic differences in I.Q. between races since Arthur Jensen started it all off in 1969 with his *Harvard Educational Review* article, "How much can we boost I.Q. and scholastic achievement?" With the exception of an occasional overt racist demagogue and a handful of critics on the left, all writers on the subject characterize themselves as socially and politically liberal; the three authors of the present work are certainly not exceptions. *Race Differences in Intelligence* is meant to be the final word of the liberal scientific establishment on the issues. Its title page announces that it was "Prepared under the auspices of the Social Science Research Council's Committee on Biological Bases of Social Behavior." It was funded in large part by the U.S. Office of Child Development, written at the Center for Advanced Study in the Behavioral Sciences in Palo Alto, and criticized by an advisory board of prominent scientists, representatives of minority groups, and "persons familiar with public policy decisions." The result is a paradigm of that academic mode of thought and expression, whose salient characteristics are: (1) a substitution of style for content; (2) a constant contradiction between various parts or between parts and the whole; and (3) a shocking disingenuousness when faced with irrefutable evidence of logic leading to uncomfortable conclusions.

The substitution of style for content is common in standard academic discourse on issues that are likely to be of any importance at all in the lives of men and women. The principle involved is that if a thing is said with sufficient judiciousness of language, with appropriate gravity and modifying adjectives, the actual content of what is being said may be zero, nonsense, or just plain untrue. One of the finest examples I have ever read of a judiciously and pompously stated contentless statement is contained in the general summary of findings on page 238 under the heading "What Conclusions are Justified": (my italics)

1. "Observed differences in the scores of members of different U.S. racial-ethnic groups on intellectual-ability tests *probably reflect in part* inadequacies and biases in the tests themselves, *in part* differences in environmental conditions among groups, and *in part* genetic differences among the groups. It should be emphasized that these three factors are *not necessarily* independent and *may* interact."

Do we really need 237 pages of evidence and discussion to come to the nonconclusion quoted above? Does "in part" mean "in large part" or "in trivial part"? When the authors try to put a little more content in the discussion of the partitioning of causes, as they do in their very next statement, we pass from noncontent to nonsense.

2. "A rather wide range of positions concerning the relative weight to be given these three factors can reasonably be taken on the basis of current evidence . . ."

This is nonsense on two scores. First, it is scientific nonsense because it is meaningless to talk about the "relative weights" of genetic, environmental, and measurement effects in the determination of the differences between two groups. Even in the sense of the analysis of variance, a difference between two means cannot be partitioned, and even if there were degrees of freedom to partition, the result would not be an analysis of the causes of differences especially in view of the authors' previous suggestion that the "three factors are not necessarily independent, and may interact."

Second, the implication in the statement just quoted is that the evidence on genetic differences in I.Q. performance between blacks and whites is ambiguous enough to allow

almost any interpretation. This is simply untrue and is directly contradicted by the authors themselves on pages 233–234 in their summary of pages 120–133. They give there five separate conclusions based upon studies of I.Q. in racial admixture. These are the only studies that would allow any estimate of genetic differentiation between blacks and whites, since estimates of within-group heritability are totally uninformative about the causes of differences between groups. All five points are in agreement that there is no detectable genetic difference. There is no association between I.Q. and possession of blood groups more characteristic of one race; there is no difference in I.Q. between mixed race and white children in English orphanages or in offspring of U.S. occupation forces in Germany; there is no evidence of increased white ancestry in very high I.Q. blacks; children of mixed parentage have higher I.Q.'s when the mother is white and the father is black than in the reverse case; and finally, the correlation between anthropometric traits and I.Q. is only .15, a remarkably low value considering the obvious social discrimination in favor of lighter skinned blacks. The *only* evidence offered by Loehlin, Lindzey, and Spuhler that there might be some "intrinsic" difference between whites and blacks is the relative stability of the difference between mean I.Q. performance of those groups in the last 50 years, despite changes in the proportion of children in school. But, as they are quite quick to point out, the groups tested were nonrandom samples and nothing is known about the *quality* of the schooling. No matter how judiciously it is phrased, the assertion that a reasonable person could conclude from the evidence that there are any significant genetic differences in I.Q. between blacks and whites is rubbish.

The second, and more important feature of the genre to which *Race Differences in Intelligence* belongs is the mass of internal contradictions which characterizes it. To understand these contradictions, we must first look at the structure of the book as a whole. It is not designed simply to report the observed results of I.Q. tests in blacks and whites, à la Shuey. No, the "Controversial Contemporary Question" to which the book is devoted is "to determine the relative contribution of genetic and environmental variation to group differences in intellectual performance" (p. 7), most specifically I.Q. differences between blacks and whites. We are immediately led to ask the question "Why do you want to know that?" and thus to the first major contradiction. On page 7 we are given the impression that Jensen's article, "How much can we boost I.Q. and scholastic achievement," is the original instigation for the study, but then on page 12 we learn that "genetic" does not mean "unchangeable," and that "Throughout this volume we will seek to remind the reader that a high level of heritability for a given trait or character is not to be automatically equated with a low level of modifiability." Putting aside the weasel-word "automatically," this is a correct and basic statement of developmental and population genetics. But if that is true, then Jensen's question cannot be answered by knowing the relative contribution of genetic and environmental variation to differences in I.Q. Our authors try to resolve that contradiction by suggesting that ". . . if we want to alter human cognitive abilities, we ought to try to learn what we can about the biological factors involved in the development of the central nervous system." True enough, but this involves a level of biological analysis that has nothing whatsoever to gain from heritability studies. Finally, they retreat to the claim that at least heritability studies will tell us whether "minor fiddling around" with environment will "pay off" in phenotype change, or whether "new ideas about environments need to be tried." But this confuses a change in the *range* of environments with a change in their *distribution*. This confusion is most specifically relevant for the case of black-white differences, where what we require

first is not new methods of schooling but an equitable redistribution of the already existent resources.

Although the authors of *Race Differences in Intelligence* promise to remind us throughout their volume that knowing the heritability of a trait does not tell us how modifiable it is, who will remind *them*? For example:

"The interesting question then is . . . 'how heritable'? The answer '.01' has very different theoretical and practical implications from the answer '.99'" (p. 74).

"As a rule of thumb, when education is at issue h_B^2 is usually the more relevant coefficient, and when eugenics and dysgenics are being discussed, h_N^2 is ordinarily what is called for" (p. 81).

"But whether the different ability patterns derive from differences in genes . . . is not relevant to assessing discrimination in hiring. Where it *could* be relevant is in deciding what, in the long run, might be done to *change the situation*" (p. 242, my emphasis).*

We are brought from this consideration to the second major contradiction of the book. We are told on pages 13 and 75 that heritability within groups has no logical relation to genetic differences between groups. But if heritability within and between groups arises from different causal and historical pathways, as they do, what is the function of chapter 4 on heritability or of the first half of chapter 5 which is devoted to differences in heritability of I.Q. within blacks and whites. The plan of the book somehow contradicts the early assertions and leaves the reader with the impression that heritability within groups does somehow tell us about the causes of race differences. This contradictory impression is strengthened by an appendix on DeFries' formula relating within and between group heritability. But DeFries himself has been careful to point out that the formula is a tautology that contains no causal information at all. Moreover, the style of statements leaves the reader with the impression that maybe there is no necessary and airtight connection between heritability and group differences, but that our old friend the "reasonable man" could safely guess that there was some genetic difference between groups if there was high heritability within groups.

If we examine the whole of *Race Differences in Intelligence*, there are really only 14 pages (120-133) that have any direct relevance to genetic differences in I.Q. performance between blacks and whites. Here information from interracial matings is used in five different ways to look for genetic differences in I.Q. between races. The conclusion is unambiguous. There is no genetic superiority of whites over blacks and indeed, as often as not, genes from black populations are associated with a slight *increase* in I.Q. performance. Loehlin, Lindzey, and Spuhler agree with this characterization of these results in their summary on page 234. Yet in their final chapter "Implications and Conclusions," they discuss seriously and in some detail what the social implications of a genetic difference between races would be, and, as we have noted above, state that "observed average differences in the scores of members of different U.S. racial-ethnic groups on intellectual-ability tests probably reflect . . . in part genetic differences among the groups." Unless they mean "in *trivial* part," this statement is at variance with the facts they cite and their own summary of these facts.

There is room for disagreement about whether the obscuring rhetorical devices and internal contradictions I have discussed simply reflect the authors' own ambivalence and

* I am indebted to Professor Arthur Goldberger of the Department of Economics, University of Wisconsin, who quite independently observed the many contradictions in the book and who sent me a long list of them, from which the three quotations above are taken.

lack of intellectual rigor in approaching this problem, or whether there has been any deliberation on their part. Unfortunately, there is less room for conjecture when we consider their treatment of some very uncomfortable material. I will give only the most egregious instance.

While heritability of I.Q. within groups is clearly irrelevant to the question treated in *Race Differences in Intelligence*, the authors regard it as important and devote considerable effort to a discussion of heritability studies. In doing so, they are forced to confront the analysis of empirical evidence carried out by Leo Kamin in his book *The Science and Politics of I.Q.* On page 85 they say "Kamin's more radical assertion of zero heritability, if substantiated, might not render this present book entirely meaningless, but it would certainly require a considerable revision of its language and point of view." Thus, they devote Appendix H to a discussion of Professor Kamin's analyses. Everyone agrees that the only clean estimates of heritability can come from a comparison of identical twins raised apart and together or other similar adoption studies. This means, in turn, that central to estimates of heritability are the only large studies of identical twins raised apart and together, those of Cyril Burt and his colleagues. Thus estimates of Burt and Howard, Jincks and Fulker, Jencks, and Morton (all cited by Loehlin, Lindzey, and Spuhler) depend critically on Burt's "data." Yet Kamin has shown unequivocally that Burt's "data" simply do not belong to what we normally think of as objective science. Among the many revelations, two stand out in particular: (1) Burt did not carry out his analyses on the results of I.Q. tests themselves but made adjustments in the test scores ("final assessments") based on his personal opinion about the differences and similarities of the twins. When twins raised together were too discordant in test scores, he adjusted those scores. (2) The correlations reported between twins raised apart (.771) and twins raised together (.944), *maintained their values to the third decimal place* in three separate studies involving different sets of twin pairs.

These revelations were so damning that Arthur Jensen himself called attention to them, among others, in an article in *Behavior Genetics* (4:128, 1974) and concluded that Burt's data could not be regarded as objective scientific evidence. Yet Loehlin, Lindzey, and Spuhler's entire characterization of Kamin's results are: "Kamin's scrutiny of Burt's published work amply demonstrates what in fact is clearly the case—that Burt's empirical studies in this area are inadequately and often carelessly reported, at least in sources readily available to the U.S. investigator."

Apparently one can be careless to the third decimal place! Not a word about "final assessments," not a hint about identical correlations in separate studies, not to mention the various discrepancies in sample sizes reported (and unreported). This attempt to cover up the scandal of Sir Cyril Burt's papers cannot itself be characterized as inadequate or careless. It is meant to shield the reader from the unpleasant thoughts that might arise about the whole field of genetic studies of I.Q., if he were to realize that its leading figure had played fast and loose with the observations and their analyses.

The failure of *Race Differences in Intelligence* to provide a hard and incisive analysis of the problem it sets itself arises from the belief structure of the scientific community which, in turn, reflects one of the guiding unexamined principles of intellectual life. It is the principle that the truth about anything always lies about halfway between the most extreme possible position. Thus, if Jensen says heritability of I.Q. is .80 and Kamin says it could be zero, the truth is probably somewhere around .4-.5.

Words like "absolute," "zero," "always," and "never" are anathema to the tradition that gives rise to this book. "Relative," "not significant," "usually," and "sometimes" are

the more comfortable, less threatening rhetoric we find in it. But the real truth about the world is different. One plus one is never two and a half. Our world of intellect is a world of two-valued logic; if A does not logically entail B, we are not allowed by some muddleheaded, middle-of-the-road ideological commitment to suggest that it sort of does, at least in months with an R in them. Sometimes even scientists tell conscious lies to make a point, and those lies cannot be made into a kind of semi-truth by describing them in an appropriately gentlemanly fashion.

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